Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at http://about.jstor.org/participate-jstor/individuals/early-journal-content.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

PROPOSED INTERNATIONAL GEOLOGIC INSTITUTE[1]

It is a source of profound regret that imperative circumstances prevent my attendance at what I am sure will be a most important session of the International Geological Congress. Forbidden this pleasure, I venture to show my interest by offering some suggestions relative to a line of effort tributary to the leading purpose of the congress. At the first session of the congress in 1878, which I had the pleasure of attending, a dominant theme of discussion was geologic classification, and this continued to be the foremost theme for subsequent sessions until it was found impossible to agree upon any system proposed. It furthermore developed that many of the most able and experienced geologists were of the opinion that it was premature to attempt any authoritative action in the matter, since in their judgment the groundwork for a *permanent* classification was not yet sufficiently broad and firm, and they felt much apprehension respecting the trammeling effects of sanctioning a premature classification. Thoughtful geologists who have given the matter careful study will quite generally agree that much is yet to be learned of fundamental facts and principles before a classification can be authoritatively adopted as the mature judgment of the geologists of the world without great risk of hampering the progress of true classificatory ideas. It seems not unlikely, therefore, that an authoritative adoption of a general classification will continue for some time to be regarded as a great end to be ultimately reached, but not wisely attempted until the foundation is better laid.

In the meantime, what can be done to hasten the great achievement?

A true classification of geologic history must represent its *natural* divisions, if there be such natural divisions. In the judgment of some geologists there are no natural divisions that hold good beyond limited provinces. They recognize no general divisions of a sufficiently definite kind to serve the purposes of a concrete classification. In their judgment the succession of geologic events was a continuous progression. They admit that it was differentiated locally and even continentally, but not so universally as to furnish a good basis for classification. They recognize that the existing classifications are natural in some degree as applied to Europe and America, the regions upon which they have been founded, but they anticipate that they will prove quite arbitrary as applied to other continents and to the world as a whole. Entertaining these fundamental views, they hold that classification should be regarded merely as a convenient arbitrary device, and that the existing systems should give way to a more convenient one, much as the old systems of measurement are giving way to the metric system.

---

[1] Presented to the eighth session of the International Geological Congress at Paris, August 1900.

On the other hand, there are those who regard the history of the earth as naturally divisible into important stages whose recognition constitutes a leading function of philosophic geology. None of these contend that there was at any time a universal cessation of sedimentation or a complete break in the continuity of life. They freely admit and affirm that there is a fundamental continuity, but at the same time they hold that progress was not uniform, but pulsative or rhythmical. Specifically, speaking for some of these, they think they find periods of stress-accumulation followed by periods of stress-relief, periods of land-expansion followed by periods of sea-transgression, periods of topographic accentuation followed by periods of base-leveling, periods of climatic uniformity followed by periods of climatic diversity, periods of biologic luxuriance followed by periods of biologic impoverishment, in short, a pulsative progress whose successive phases furnish a natural basis for classification.

The existence of these diverse views is an expression of the imperfection of present knowledge. Were exhaustive data at command it could be determined whether the dominant character of the earth's progress was uniformity or periodicity, and hence whether we should primarily seek a scale of reference and nomenclature with a uniform arbitrary unit, as the meter or the century, or whether we should strive to measure progress by its inherent waves or nodes, or whether we should seek both impartially.

If the rhythmical view be the most laudable, effort should be directed to the more complete and accurate determination of the nature and limits of the periodicities, and to the modification of present classification, so as to bring it into more complete conformity to these. If the uniformity view be the more laudable, effort should be directed to reducing the adopted divisions to quantitative equality by perfecting the geological column and determining available scales of measurement, both stratigraphic and chronologic.

In either case, or in any other case which any individual geologist may prefer to put in the place of these selected ones, it is necessary to push investigation a long way forward before an International Congress can wisely give its sanction to any specific classification.

Our shortest road to the great end sought will therefore be found in promoting those investigations which will soonest give the needed groundwork. Fortunately these investigations are precisely those which best subserve the higher philosophic purposes of the science. Two phases of this great work stand forth prominently: (1) The systematic compilation and elaboration of the great mass of data produced by studies in all parts of the world, which are not now fully available, save to a few favored workers connected with the great libraries, and to these only through much labor duplicated in every individual case. It will be admitted without discussion that the collocation and organization of existing and forthcoming data would greatly stimulate accretions and promote the end sought. (2) *The development of additional criteria of correlation. A preëminently essential step in the progress of classification and interpretation is an increase in the precision and the certainty of correlation of formations in widely separated regions.*

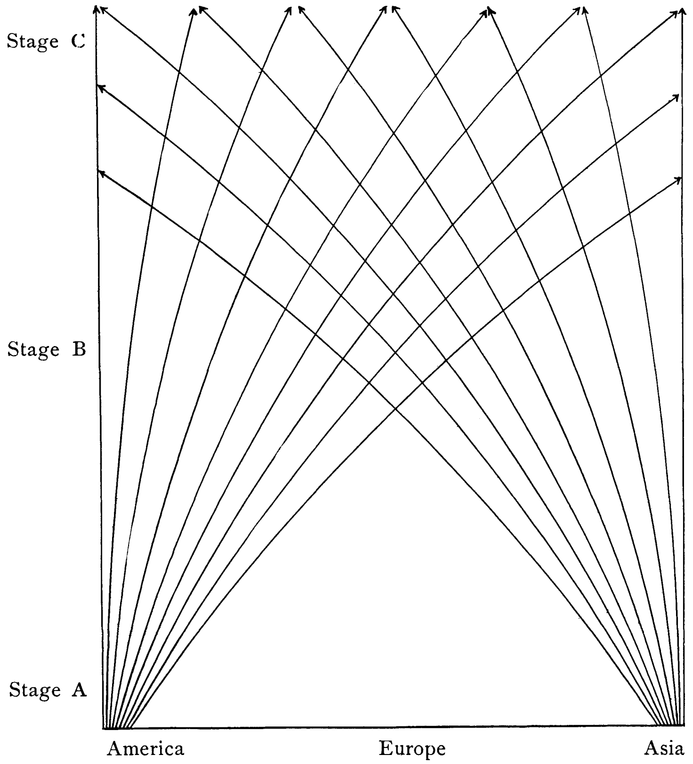
At present the general conviction prevails that there is but a single trustworthy basis of correlation between separated regions, and that even this is subject to some important qualifications, and in many cases is wholly unavailable. I need not mention fossil contents. But while fossils are, and apparently must ever remain, the chief means of correlation, it has been the growing conviction of my recent years that certain important improvements and extensions of the criteria of correlation are possible. These embrace (*a*) an improvement of the paleontologic criteria by correcting them for the uncertainties and errors introduced by migration; and (*b*) the addition of physical criteria to the paleontologic ones in such a way as to eliminate some of the uncertainties of the latter, and to serve in their place when they are not available, as is so often the case.

It is neither appropriate nor possible to set forth these adequately here, but I should fail to duly magnify the importance of the measure herein advocated if I did not at least try to indicate the greatness of the possibilities which we might hope to realize if we had the means at our command.

(*a*) The line of improvement in paleontologic correlation may be best illustrated by a concrete example. Let it be supposed that a provincial fauna arises in a harbor of refuge during a time of sea-withdrawal from the continental shelf[1] on the border of America in a given stage A; that by the subsequent development of an adequate sea-shelf or a connecting series of epicontinental seas, this fauna migrated to the shores of Europe during stage B, and that ultimately it reached the coast of Asia in stage C. By the simple application of the criterion of community of species, stage C of Asia would be correlated with stage A of America, and although the time-interval might not be very great, the error growing out of it might seriously disconcert the correlation of associated physical events, and prevent their correct interpretation. But if during the stage A there developed in a harbor of refuge on the coast of Asia another provincial fauna, and if this, for like geographic reasons, was permitted to reach Europe in stage B, and America in stage C, a different set of errors of interpretation would be likely to arise from the simple criterion of community of species, for the commingled faunas would appear in America and Asia only in stage C, while they would appear in Europe as early as stage B. So too, in America and Asia, in stages A and B, only the respective provincial faunas would be found. The situation is illustrated in the accompanying diagram. But if the fact of the provincial origins of the two faunas and their subsequent migration can be established, a much more accurate correlation will be possible. The deposits of stage A in Asia and America will be correctly

---

[1] "A Systematic Source of Evolution of Provincial Faunas," JOUR. GEOL., Vol. VI, No. 6, Sept.-Oct. 1898, pp. 604–608.



assigned to the same period, though they have no species in common, and the other stages will fall into their proper sequence. This may suffice to illustrate one method by which the migration, *and particularly the cross-migration*, of faunas may be brought into service in the correction of the errors of correlation arising from dependence simply on the presence of common fossils. It will furthermore be recognized with acclaim that the tracing out of the origins and migrations of the ancient races of the earth's inhabitants has independent and profound interest, and that it is peculiarly and necessarily an intercontinental work.

But the origin and migration of faunas and floras is peculiarly dependent on physical conditions. I am persuaded that this is most eminently true of the origin of provincial faunas and their evolution into cosmopolitan faunas, as I have endeavored to set forth in recent papers.[1] While it will doubtless always be quite difficult to detect the point of origin and the course of migration of *single species*, there is reasonable ground of hope that the origin of provincial *faunas* may be located, and their migrations and fusions followed until they are lost in cosmopolitan faunas. I have given reasons elsewhere for believing that the general production of provincial marine faunas of the shallow-water type is connected with the withdrawal of the sea from the upper face of the continental platforms, associated with surface warpings, by the first of which the areas of shallow water are restricted, and by the second dissevered from each other.[2] It is therefore believed that much aid in rendering paleontologic interpretations more significant, more certain, and more precise may be derived from the study of the bodily movements of the earth and of the evolution of the geography of the continents in their migratory relations. Certainly and admittedly this is eminently true of land faunas whose migrations are essentially dependent on terrestrial connections, and I believe it is scarcely less true that the migration of the more immobile portion of shallow water sea faunas is dependent on continental shelves and epicontinental seas. If, therefore, there be a periodicity in the great bodily movements of the earth and sea, and a consequent periodicity in the origination of provincial faunas, there will be all the greater field for the application of principles founded on migration and counter-migration to the working out of more precise correlations.

---

[1] "The Ulterior Basis of Time Divisions and the Classification of Geologic History," JOUR. GEOL., Vol. VI, No. 5, July-August 1898, pp. 449–526.

"A Systematic Source of Evolution of Provincial Faunas," JOUR. GEOL., Vol. V, No. 6, Sept.-Oct., 1898, pp. 597-608.

"The Influence of Great Epochs of Limestone Formation on the Constitution of the Atmosphere," *Ibid.*, pp. 609–621.

[2] The first two papers above cited.

(*b*) There is reason to hope that the sea itself may be made an important aid in intercontinental correlation, for it makes, and always has made, *a simultaneous record on all the continents.* The difficulties lie solely in reading the record.

The ocean volume may not have been accurately constant at all times, but its variations between closely related periods can never have been more than a negligible fraction of the whole volume. Its record is made on the border of a single complex basin, for all the oceans are united and have a common water level. Apparently there has always been essentially a single complex basin, though this cannot be rigorously affirmed. Assuming it, however, any deformation of the basin in any part, which affects its capacity, is recorded on all continents by a new shore line. Setting aside compensatory warpings, this involves a universal advance or retreat of the sea, and a corresponding change in the areas of sedimentation and erosion. It involves, also, a change of land and sea faunas by expanding and contracting their habitats respectively, and by extending or restricting their means of migration. Were it not for the attendant warpings of the land, these sea changes would furnish a simple means of world-wide correlation of the most precise and specific kind. Conjoined with paleontology they would leave little to be desired so far as marine stages are concerned. There remains, however, the problem of eliminating the disturbing effects of concurrent warping. To some large extent it is the warping that changes the capacity of the ocean basins. Warping may even disturb the local attitude of the land to the sea without affecting the capacity of the ocean basin, for the warping up in one region may be compensated by the warping down in another region. The subject is therefore attended by grave difficulties, but it is not without sufficient ground of hope to justify systematic study. There are at least two general phases of earth-action whose effects promise to rise above those of local warpings to such a degree as to be determinable and to be serviceable in intercontinental correlation. They are the stages of great shrinkage and the stages of relative quiescence. Whatever views may be entertained of the early history of the earth or of its internal constitution, it will probably not be questioned that the oceanic bottoms have, on the whole, shrunk more than the continental platforms. The very existence of the continents in spite of erosion is an expression of this. It will perhaps not be denied that the shrinkage adjustments of the exterior of the earth have been periodic and that the basins have been deepened and the land relatively elevated at the periods of adjustment; at any rate, this may be made a working hypothesis, coördinately with its opposite, until the truth is ascertained.

Between the assumed periods of adjustment, periods of relative quiescence may be recognized as their necessary complement. These were only relatively quiescent, for local and regional warpings were quite certainly in progress at all stages of geological history. In these stages of relative quiescence the volumetric erosion of the land may quite safely be assumed to have exceeded the volumetric elevation, and the material transported to the sea may be assumed to have exceeded any increase of the capacity of the ocean basin due to shrinkage. Without these assumptions it seems to me difficult to explain specifically the history of erosion and sedimentation; but, as this may be doubted, let the assumptions stand merely as working hypotheses. The result of such erosion would be the partial filling of the common ocean basin and the extension of the sea upon the land. Taking Murray's estimate of the present average height of the land, a volumetric reduction and transference of one half the protruding portion would raise the sea level somewhat more than one hundred meters, an amount sufficient, in the lowered state of the continents, to notably extend the marine area and change the distribution of life.

Here, then, hypothetically are two systematic causes tending to produce universal changes in the relations of sea to land, the one dependent on the accumulation of shrinkage stresses by reason of the effective rigidity of the earth, and the other dependent upon erosion during relatively quiescent stages. Now, if by the compilation of great masses of data the disturbing effects of local warping can be eliminated, a means of intercontinental correlation, independent of the paleontologic, and fundamental to it, may be obtained. By combining the dynamic with the paleontologic the testimony of both may be enhanced and the significance of the latter greatly increased.

The application of the method obviously requires the massing and handling, quantitatively as well as qualitatively, of all possible data from all parts of the world, and can only give results of the highest order of trustworthiness when the mapping of the whole earth approaches completion, but there is reason to believe that initial results of no small value might even now be secured if all existing data could be marshaled.

The constitutional states of the atmosphere furnish a third source of hope of supplementary means of correlation. If the view that the atmosphere originally contained all or the larger portion of the elements which have been taken from it, notably the carbon dioxide, and that its history has been one of continuous depletion, and that, aside from a slow decline in temperature, climatic changes have been due merely to local agencies, be correct, there is little ground of hope for results of much value in correlation. But if, on the other hand, as recently postulated,[1] the carbon dioxide of the atmosphere has been mainly supplied concurrently with its consumption, and the amount has varied with the ratios of supply to consumption, and these ratios have been dependent upon the extent of the land and the condition of the sea, and if further the impoverishment of the atmosphere in carbon dioxide is determinable by the unusual prevalence of certain kinds of deposits, as salt, gypsum, red clastics, and glaciated bowlder clays, while its enrichment is indicated by equable temperatures and mild climates in high latitudes, there is ground for hoping that the constitution of the atmosphere may be made to afford a valuable means of correlation applicable when the paleontologic criteria are most liable to fail. Effects due to the constitution of the atmosphere must be universal and strictly simultaneous, though of course not everywhere identical. For example, in India, Australia, and South Africa, an abrupt change in the flora took place at some time near the transition from the Carboniferous to the Permian period. Fontaine and White have found an abrupt, though less radical, change in the flora of eastern America at about the same time, but there are no paleontological means at present known by which these changes can be accurately correlated. The change in India, Australia, and South Africa is closely associated with glacial deposits. Now, if glaciation be a result of a *constitutional* state of the atmosphere, such state should make itself felt *in all parts of the earth simultaneously*, though in different ways and degrees, and the floral change in eastern America could with good reason be strictly correlated with the changes in Asia and the southern hemisphere, and should find verification in similar changes elsewhere. This is merely a hypothetical illustration. The supposed atmospheric mode of correlation should be verifiable by its peculiar effects, for it should simultaneously affect distant floras made up of different constituents, a phenomenon differing in nature from the effects of simple migratory replacement or biologic evolution.

---

[1] A Group of Hypotheses Bearing on Climatic Changes. JOUR. GEOL., Vol. V, No. 7, October-November 1897.

The Influence of Great Epochs of Limestone Formation on the Constitution of the Atmosphere, JOUR. GEOL., Vol. V, No. 6, September-October 1898, pp. 609–621.

An Attempt to Frame a Working Hypothesis of the Cause of Glacial Periods on an Atmospheric Basis, JOUR. GEOL., Vol. VII, Nos. 6, 7, and 8, 1899.

It is obvious, however, that correlation by atmospheric states can only become a trustworthy dependence by the massing and careful adjudication of data from all parts of the world, for the sanction of the method will largely depend on its success in conforming to and elucidating world-wide phenomena. Both this and the preceding method will require first to be established by trial before they can be independently applied, but this was equally true of the paleontologic criteria at the outset.

The very establishment of these atmospheric and oceanic criteria, or their disproof, would go far to settle the fundamental question whether the earth's history is naturally divisible into periods or not. If there be secular accumulations of stress in the body of the earth, followed by adjustments when rigidity is overcome, and if these adjustments change the respective areas of land and sea, and these in turn result in changes in the constitution of the atmosphere and in the evolution of life, these cycles must be factors in the ultimate basis of a rational classification of geologic time, and must at the same time express some of the most profound and significant features of the earth's history.

The doubt as to whether these things be so or not can only be resolved by studies as broad as the earth. Such studies are not only international and intercontinental, but they are omniterrestrial. The shaping they have been given here is that of an individual student and expresses his limitations, but the breadth and importance of the problems themselves will not be questioned.

Now it is far beyond the functions and the resources of any present official organization to adequately cope with these great problems. There is not even an organization at present that is provided with the men and the means to bring promptly together, arrange, and tabulate for the common benefit of geologists the data that are being gathered by official and private investigations; much less is there any organization that can build these into their organic relations, or draw forth from them their full significance and make this serviceable in further investigation. Nor is there any organization provided with any notable means for systematically supplementing the work of official and private surveys where such auxiliary work is most needed. A few universities and a few generous individuals provide means for particular expeditions and these have great value and should command our profoundest appreciation, but they are not, and cannot be expected to be, systematically related to classification or to the other general questions under the special patronage of this body.

I beg to urge, therefore, upon the International Congress the inauguration of a systematic movement looking to the determination of the fundamental facts upon which an ultimate classification may be founded.

The practical suggestion I ask leave to offer is an appeal to the generous people of our several nations for the means to establish a permanent institute, or group of coördinate institutes, which shall be devoted to this purpose and to others germane to it. Specifically these purposes may be summarized as follows:

1. The collocation, systematic arrangement, and publication of existing and forthcoming data bearing upon the fundamental facts and principles upon which a final classification must be founded.

2. The gathering of literature, especially that now least accessible, and, so far as practicable, of typical collections bearing on the special objects of the organization.

3. The elaboration of data by combination, computation, and correlation, both independently of all hypotheses and in specific application to the various hypotheses that may be propounded.

4. The encouragement, and so far as practicable, the conduct of field investigations in regions not cultivated by existing organizations, nor reached by private enterprise.

The magnitude and duration of the work would require that the institution be permanent. If the task of the complete correlation of the earth's formations be assumed it is doubtful if the function would ever be entirely fulfilled.

The generosity with which great institutions of learning have been founded and expensive scientific expeditions equipped gives encouragement to the hope that if the noble object were suitably made known, the means for its realization would be forthcoming.

It may seem premature to discuss details of organization and modes of control before the general nature of the proposition is approved, or substantial encouragement be given that the necessary means of carrying it into effect will be available, but the practicability of the proposition is in some measure dependent on the concrete form which it would take. This is a peripatetic body of changing membership, and, so far forth, is not most happily organized to administer an enterprise of this kind. The congress, furthermore, represents many nations and could not be hoped to be entirely at one as to the location and control of a permanent institution of this kind. It is, furthermore, probable that patrons of the proposed organization would be influenced by patriotic sentiments in making contributions to the endowment. It seems best, therefore, to recognize these conditions in the proposition itself, and instead of endeavoring to establish a single international institution under the specific control of this body, to urge the establishment of sections or branches or coöperative members of a composite organization to be located in as many nations or grand divisions of the globe as future developments might render wise; these sections or branches to be immediately administered by such bodies in such nations or grand divisions as were found most suitable and practicable. For example, in America, a section or a coöperative institution might well be established under a board of trustees elected by the Geological Society of America, a permanent organization of definite membership, representative of geological activity on the North American continent. Similar representative societies or fixed geological organizations fitted for the control of other branches or coöperative institutions exist, or if not, should be brought into existence, in all the great nations or grand divisions. Such regional organizations would possess certain advantages in the collection of data from the fields they would specially represent. The function of the International Congress would embrace the inauguration and coördination of these, the advisory control of their lines of effort, the adoption of their partial results as reached from time to time, and the creation of international sentiment in their support.

T. C. CHAMBERLIN.

UNIVERSITY OF CHICAGO,
July 25, 1900.